

DISCUSSION BEFORE THE WIRELESS SECTION, 3RD FEBRUARY, 1937

Dr. R. L. Smith-Rose: In Mr. Hill's introductory remarks it is stressed that the specification has been prepared by manufacturers for manufacturers. What is ultimately required, however, is an agreed specification, prepared by a representative body of not only manufacturers but also users and any independent authorities;

in fact, we want a Standard Specification, possibly of the present type but sponsored by an independent responsible body. When I raised this matter at the discussion on Mr. Thomas's paper* in 1932, it appeared that my sug-

* *Journal I.E.E.*, 1932, vol. 71, p. 128; and *Proceedings of the Wireless Section*, 1932, vol. 7, p. 120.

gestion was a little premature. I am going to suggest that the time for the preparation of a Standard Specification has now arrived, seeing that the technique of broadcasting-receiver development has reached a more or less stable condition, particularly as regards the long- and medium-wave broadcast bands. Mr. Hill's remarks contain no indication that a Standard Specification is contemplated by the R.M.A., and his final sentences envisage the fact that, after accepting the criticism which this meeting and other publicity given to the specification may produce, the manufacturers will themselves consider whether it will be necessary for them to review and revise their own work. It is therefore for the manufacturers to take the initiative in this matter, and make a definite approach to the British Standards Institution.

Mr. M. G. Scroggie: With regard to what Dr. Smith-Rose has said about the proposed specification being sponsored by an official, independent body, it would be necessary to see that this proposal did not lead to undue delay. Some years ago, when ebonite panels were being standardized, by the time the specification was finished ebonite panels had long since become obsolete.

Referring to Part I, Section 2, paragraph 1.2 (long-waveband sensitivity measurements), I deprecate the choice of 200 kc. as a measuring frequency, because, unless one makes the measurements in a screened room, Droitwich is liable to leak through sufficiently to introduce difficulties owing to beat notes. The frequencies I have found most suitable in practice are 175, 225, and 275 kc., and these have the advantage of being evenly spaced over the scale. As regards the method of expressing the sensitivity results (paragraph 2.1), I take it that if a smooth curve is plotted the three specified points will not be assumed to be adequate data for the purpose.

In Part I, Section 3, of the specification it is stated "this measurement applies at present only to long- and medium-wave bands." Is there any practical objection to applying it to short-wave bands? I take it that it is intended later to extend this work more fully to those bands. I should like to suggest that it is better to express the results of selectivity measurements in decibels, instead of as "the strength in millivolts of the unwanted carrier." Decibels are more appropriate, seeing that selectivity is essentially a ratio; and this is implied by specifying 40 db. in paragraph 1.2. An additional reason is that the output power specified for this measurement is different from that for sensitivity.

In Part I, Section 4, paragraph (1.1), it is stated that the r.m.s. noise output is measured. I think that in almost all cases a metal-rectifier output meter is used, which of course does not measure r.m.s. values, and the error in the case of wave-forms such as are in view is considerable. In paragraph 1.4, why is 100 cycles per sec. selected for the filter? If it is to remove hum, this frequency is too low to do so effectively.

With regard to automatic volume control, the method described is open to the objection that if one starts with the maximum signal at 1 volt (incidentally, not all modern generators give a full volt), adjusting the output to a quarter of the nominal power output, the outputs at the smaller signals may be too small to measure; and this region of the curve may be important. Also the

method fails to show whether the delay voltage is correct. The method that I have adopted is to begin with the smallest signal, with the volume control at maximum. Then, when the output reaches such a point that overloading is within sight, e.g. at one-quarter of the nominal output, the volume control is turned down until the output reading is reduced to one-tenth. If necessary, the process is repeated at a still higher signal voltage. This method sometimes gives apparent output figures considerably in excess of the nominal output for the receiver, which is an indication that the delay voltage is excessive. I think this point is rather important, because the ease of handling of the volume control depends very much on the range of volume available being correct.

I see there is no reference in the paper to quiet automatic volume control (Q.A.V.C.). I take it that the same method would be used, but I should point out that it is necessary, in plotting a curve of Q.A.V.C., to take it both with increasing and with decreasing inputs, because sometimes the curve forms a loop. The specimen curve* obviously shows the ordinate scale in volts instead of watts as specified.

With regard to Part II, in Section 1 it might be desirable to correct the expression "equally as loud as the signal," which is not good English. Also, with regard to the words "a note which an average listener judges to be equally as loud as the signal," surely this is a matter of enormous difficulty. If one has to compare a note of 1 000 cycles with one of, say, 50 or 5 000 cycles, it is almost impossible to judge even approximate equality of loudness.

I am glad to see that a real attempt has been made to tackle the measurement of hum. I also notice that Mr. Hill realizes the difficulties, e.g. that apparatus which works perfectly quietly on mains in one area may emit an intolerable hum elsewhere.

With regard to the measurement of acoustic output, it is difficult to comment on this in the absence of practical experience, but the specification seems to embody a courageous attempt to solve an extremely difficult problem. One realizes this when even the somewhat complicated method described yields only very partial data. The curves ought really to be extended to several other degrees of freedom—the whole audio-frequency scale instead of one point; the whole range of modulation depth instead of one point; and actual (reactive) loads instead of an artificial load. All this would naturally make the work far too laborious for practical purposes.

Dr. S. Whitehead: I should like first to refer to one or two details of technique in respect of which the specification may require modification.

The use of two signal generators in series involves some care, and the American specification—rightly, I think—leaves a good deal to the judgment of the operator. The British specification is more detailed, to the point of being somewhat restrictive, without dealing with all the difficulties which may beset the inexperienced. At the higher-frequency end of the band covered, the shielded-inductor method may be troublesome. Precision field-strength sets usually employ a direct coupling, but even so do not give very good comparisons. The E.R.A. have found it convenient to employ the radiation field

* Not reproduced here.

of a loop carrying a known current for the calibration of receivers with frame aerials at higher frequencies.

The possibility of avoiding the 1 500-cycle reference tone in addition to the 400-cycle reference frequency which itself is, I believe, mainly confined to radio-receiver practice, is deserving of consideration. The difference between the C.C.I. standard frequency of 800 cycles per sec. and the acoustic reference frequency of 1 000 is unfortunate, and any simplification of standards in other fields would assist the attainment of a single ultimate standard tone. Similar considerations apply to the assessment of a complex wave. Different circumstances require different types of weighting for various components, but in the absence of a comprehensive statistical analysis for the specific problem (such as the psophometric voltage defined for telephone practice by the C.C.I.) it would seem better to select such of the existing well-known weighting curves, of which there are several, as seems the most appropriate. For example, the weighting curve for measuring hum in the present specification corresponds closely to that adopted by the C.C.I. for disturbance to broadcast transmission over telephone channels, except that the curve for hum cuts off at 800 cycles per sec. Again, distortion appears to be assessed both by the "harmonic content," which is the same as the "harmonic residue," a term now of some years' standing, and also by a weighting proportional to frequency. The distortion of the electric supply is represented by the addition of 3 per cent 5th harmonic, which is somewhat arbitrary in view of the amount of work that has recently been carried out on this subject in Europe and America and in which the commoner types of distortion have been analysed and to some extent defined. One might envisage the expression of component quantities in such a way that they would represent a simple subtraction from or addition to the fidelity of the transmission as a whole, so that the special and subjective features of this particular service would be included in a single unit to which others could be related, as is attempted in telephone practice. Be this as it may, the question of appropriate standards supports the later intervention of the B.S.I., suggested by Dr. Smith-Rose, since the B.S.I. acts as the National Committee of the I.E.C., which, as a standardizing body, is attempting to organize some international agreement in the definition of acoustical and associated quantities, which are becoming of such importance in electrical engineering.

Finally, I hope that a section on the assessment and reduction of the liability of a receiver to interference will later be included in the specification. Screening, mains filters, design of aerials and earths, and many other features, can serve to reduce interference. At a time when specifications are in preparation for the reduction of interference from electric appliances and plant, corresponding directions to the users of this specification would have a valuable influence. Whatever the eventual amelioration of the situation may be by other means, it is probable that appropriate measures at the receiver will be necessary in some unfavourable cases, where the full advantage is desired to be taken of the progress in receiver performance, a progress to which this specification is itself a witness.

Mr. E. L. E. Pawley: It is an unfortunate fact that

many listeners are quite as happy with very bad quality as they are with technically excellent quality. I think we must admit, however, that a discriminating listener has a right to a receiver which will come up to his requirements, and that all manufacturers would like to be in a position to say amongst themselves whether or not their receivers fulfil those requirements.

The ideal solution of the problem is to relate the radio-frequency energy in the aerial to the acoustic effect on the listener's ear, because his ear and the corresponding effect on his brain are the only things that ultimately matter. There are, however, two very great difficulties in making these acoustic tests. One is the cost of the damped room, which may have been rather underestimated, because it is difficult to construct a satisfactory damped room for measurements going down to 150 cycles, and to get down to 30 cycles is extremely expensive, if not impossible. The other point is that it is necessary to allow for the acoustics of the listener's room, which are in general not known. This is particularly difficult in the case of broadcasting receivers used in schools, where there is a considerable distance between the listeners and the loud-speaker, and the proportion of reverberant to direct sound is unusually high; and consequently the total spherical output of the loud-speaker may be considerably more important than the output in one particular direction. The specification tries to get over this difficulty by stating that the output shall be measured in two different directions, namely one along the axis and another at an angle of 45° to it; but I am not sure that this provision will be adequate to cope with cases, such as I have mentioned, of broadcast receivers for use in schools.

It is interesting to compare the methods given in the specification with a method which has lately appeared in one of the French technical papers, and a similar one which has recently appeared in the German technical Press. In general the fundamental theory is very similar, but I have noticed one or two differences. In the first place, the average listening level for the sensitivity tests is taken in both the foreign methods as between 60 and 74 phons, whereas the R.M.A. specification mentions 50 phons. Another point is that the depth of modulation which is taken as standard in the R.M.A. method is 30 per cent, whereas the French prefer to use 80 per cent—probably unwisely!

There is one respect in which the specification might possibly be simplified. The formula given in Part II, Section 3, for converting the measurement of electrical sensitivity into the figure for acoustic sensitivity is not quite the sort of thing which the average engineer would use if he could help it. Would it not be very much simpler for the average man who is making a test on a broadcast receiver just to repeat a portion of the frequency curve under the conditions which are specified for the measurement of sensitivity, so that at one stroke he gets a final figure for the result instead of having to combine together three different factors in order to arrive at it?

I should welcome some more definite recommendations on several important subjects which are not adequately treated in the specification. The first is second-channel interference, which can be very important, especially in

short-wave reception. The second is the possibility of non-linear distortion in the loud-speaker, which is not covered because it is very difficult to measure. I feel, however, that an attempt should be made to include it, bearing in mind the possibility of difference-tones and modulation products being actually of more importance than the harmonics which are measured in accordance with the specification. The third is the stability of the receiver and the accuracy of the frequency calibration.

There must be a certain reluctance to specify definite limits for individual characteristics when the overall performance of a receiver is an unknown combination of them, but I think we can assume that if all the receivers produced conform in every respect to a certain specification, and if we can say that 99.9 per cent of the people who purchase them and use them in their rooms, whatever they may be like, are satisfied, then we may consider that the specification is a satisfactory one.

Dr. L. F. C. Hughes: The figure of 50 milliwatts for the standard output seems low for present-day broadcast receivers, battery sets with an output as low as 400 milliwatts being rare. Also the output impedance of the signal generator is assumed to be so low that the output voltage in the specified measurements is independent of the impedance of the load. In rating the power output of radio receivers, what percentage harmonic distortion is taken? In Class B battery sets the harmonic distortion generally drops to a minimum which may be greater than the accepted harmonic distortion for rating purposes.

In measuring receiver noise the output meter must be a square-law rectifier for the specified indications to be made. If the meter is of the metal-oxide rectifier type, which indicates the average of an applied wave-form, the indication with the two powers, one a 1 500-cycle tone and the other random noise, may not be the sum of the two powers measured separately. A simple calibration or calculation will, however, obviate this disadvantage. The zero of the British Standard phon scale is 0.200 millidyne (r.m.s.); the American standard is 0.204, but the difference is acoustically negligible. The level to be taken, i.e. 50 phons, is certainly too low. The level of my lecturing voice (average continuous r.m.s.) in a medium-sized classroom is about 84 phons, the occasional peaks reaching 91 phons. Allowing only 10 db. drop for a conversational level, this agrees with the figure (74 phons) given by the German specification as representative. With the normal technique of control, music will probably rise occasionally to 80 phons, which is not unreasonably representative of a large orchestra. The distortion factor, in taking into account the order of the harmonic, is a great improvement and will indicate a truer measure of amplitude distortion experienced with Class-B and pentode output stages, the use of which has greatly deteriorated the general average of reproduction obtained nowadays.

A more convenient way of expressing the amplitude distortion is to use the number of decibels the weighted power of the harmonics is below the wanted fundamental. This is representative of the blur level, i.e. the level of those alien frequencies which blur the required frequencies. The blur levels so obtained could then be

compared with the levels obtained subjectively with other types of blur, such as are provided by reverberation. The extent to which a given distortion factor or blur level degrades the quality of reproduced sound can only be learned by subjective tests. Insufficient tests have been made to indicate the relative amount of the several classes of blur which are just detectable, or how much of one is equivalent to a given amount of another.

With regard to the radiation of sound from a loud-speaker, some experiments I made 10 years ago indicated that the response of a coil-driven cone mounted in a large baffle in a direction making 45° to the axis is substantially equal, within a few decibels, to the mean spherical response, which determines the average reverberant intensity in an enclosure and which determines the loudness except in the vicinity of the loud-speaker and for high frequencies, which are sharply focused into a beam. In expressing response curves, the ratio of the scales should be stated so that one can learn the meaning of response shapes by experience; 20 db. per cycle (i.e. a frequency multiple of 10) is convenient and is not acoustically misleading.

The Apparatus Approval Sub-Committee of the Central Council for School Broadcasting agrees with me that the ultimate test is a listening test under standard conditions, but appreciates the present specification as providing data, when it is used, respecting the standard to be expected in receivers for school purposes. It is unfortunate that the public never has the opportunity of hearing radio receivers under standard and strictly comparable conditions, so as to assess practical values. It is hoped that the application of the specified tests will indicate in some way a new commercial valuation of radio receivers for broadcasting.

Mr. C. N. Smyth: While I am appreciative of the value of the specification I am not quite certain that the tests for selectivity and sensitivity bear the simplest possible relationship to what the user hears, and I wish to suggest two small modifications which I think would overcome any criticism.

First, if according to the specification the sensitivity of a receiver is 1 or 2 microvolts the application of such a signal to the receiver might reasonably be expected to be able to produce an audible and intelligible output; but I do not think that in every case it would, because 50 milliwatts is frequently too small in comparison with the noise level of the receiver, or the input is too far below the level at which the muting action is released. I would prefer for these reasons to see the sensitivity measured as the input required to produce one-quarter or one-half the maximum rated output of the apparatus.

Secondly, with regard to the measurement of selectivity, in the past engineers have had to contend with four difficulties: (a) During the period of measurement the detector was not operating under the conditions obtaining in practice. (b) The A.V.C. action was liable to influence the results. (c) The audio-frequency filtering in the receiver was not taken into account in the measurement. (d) Effects of loud-speaker response on the effective selectivity were not included. Part 1 of the specification is not concerned with acoustic tests, and the new method of measurement overcomes the difficulties under (a) and (b) but does not take into con-

sideration the fidelity of the audio-frequency stages of a receiver, where an appreciable fraction of the selectivity of a modern receiver is frequently located. I think, therefore, that it would be better, when measuring selectivity in the manner described in the specification, to have both signal generators unmodulated and to obtain the standard output at the frequency of the beat note between the two incoming signals, rather than to measure only the cross-modulation component.

Mr. A. J. Gill: I quite agree with Dr. Smith-Rose that what we must ultimately hope for is a British Standard Specification. The subject of this discussion is "A Specification for Testing and Expressing the Overall Performance of Radio Receivers," and it really only applies to one special type of radio receiver, namely a sound-programme receiver. Mr. Hill hopes that the specification will be used by Government Departments; but the specification only specifies the selectivity between 150 and 2 000 kc., i.e. in the broadcast band, and Government Departments do not buy large numbers of broadcast receivers. Also in Part I, Section 2, it is said that measurements shall be made "at one frequency in the middle of each of the accepted short-wave bands." I would point out that the bands quoted in the specification are accepted for broadcasting only.

There is apparently nothing in the specification which calls for measurements to be made, on superheterodyne receivers, at the intermediate sideband frequency. The curves only show a frequency about 15 kc. off the carrier for the maximum measurement. Some measurement ought to be made on superheterodyne receivers at the intermediate frequency, because I have known such receivers to be useless owing to the fact that an intermediate frequency had been selected which happened to coincide with the carrier frequency of some powerful station, and, as these sets were insufficiently screened, that frequency went through and swamped the rest of the output.

With regard to the remarks that have been made about measurements in reverberant rooms, one cannot blame a loudspeaker for imperfect performance in a reverberant room, and while we have reverberant rooms we shall have to put up with this difficulty. After all, we tolerate speech and music originating in such rooms, although such sounds must also be subject to distortion due to reverberation.

Mr. R. M. Barnard: The specification does not mention methods of measuring phase-shift. This measurement is important for two reasons: First, the output of a radio receiver which is used for sound reproduction always contains a certain amount of harmonics, and the importance of these harmonics depends to a certain extent on their relative phase. Secondly, in receivers for television purposes (presumably the specification would cover that type of receiver) phase is of very great importance, and I think that in a future edition of the specification a method should be suggested whereby the phase-shift through a radio receiver could be measured, particularly over the frequency band required for television.

A method of measuring phase-shift has been described by Peterson, Kreer, and Ware,* but in their case the

phase-shift had only to be measured at one frequency; that is to say, it was only the comparatively simple problem of measuring phase-shift through an amplifier. In the case of a radio receiver the problem is considerably more difficult because this incorporates a frequency-changing valve, which, in theory, produces an output containing frequencies of the same relative phase as the original sidebands.

I think that in addition a figure should be suggested for the degree of phase-shift that is permissible through various types of radio receiver, depending on the purpose for which each is intended.

Mr. W. K. Alford: I have been asked to make a few remarks on the general attitude of the Army Signals authorities to the application of the specification.

After examining it fairly comprehensively we find we can give it our general approval. Unfortunately, a specification must be a very powerful weapon for both the Army authorities and the manufacturers, but it is the desire of the former that it should be a means of bridging over the difficulties which lie between the production as understood by the manufacturers and the special requirements which the Army must, of necessity, call for.

As regards selectivity, the Army requirements are vastly different from those necessary for ordinary broadcast reception, and we should require a much firmer qualification of the conditions of selectivity as outlined in the specification. In other words, we should call for a specification of the "cut-off" slope of the resonance curves in some arbitrary units such as decibels per kilocycle per sec., and we should require to know what happens outside the immediate neighbourhood of the resonance region. Further, the use of two signal-generators for the determination of selectivity would not be viewed with approval by the Army authorities. Another important point which would require attention is that a figure of sensitivity and selectivity would have to be arrived at for C.W. working as distinct from radio-telephony. As regards second-channel and other spurious forms of interference, here again considerable care is called for in drawing up the specification so that these points may be adequately covered. This matter assumes considerably greater significance when it is realized that in the future the use of the double superheterodyne will probably become more common, and in such a circuit there are great possibilities of a multiplication of spurious heterodyne frequencies. Frequency stability (particularly high-frequency) would also require qualification. Army wireless receivers are in most cases battery-driven, and most careful scrutiny is required to see that complete stability of performance is assured under all conditions. The calibration accuracy must be of the highest order for signalling purposes, and a definite standard of accuracy of calibration would be called for and expected.

Mr. Charles Holt Smith: The first thing that strikes me in reading this specification is the indecision regarding the accuracy with which the measurements are to be made. For example, in Part I a signal generator is called for with an accuracy of ± 10 per cent, whereas in Part II a signal generator is required of an accuracy of ± 20 per cent. Also in Part II, Section 1, a microphone is required to be calibrated to an accuracy of 10 per cent, whereas in Section 2 it is stated that the microphone in

* *Bell System Technical Journal*, 1934, vol. 13, p. 680.

conjunction with an amplifier should be accurate to 0.5 db., i.e. 5 per cent. Again, in Part I we find that when we are measuring the sensitivity of a frame-aerial receiver the coupling coil which is placed near the centre of the frame must have an impedance of, say, 20 times the open-circuit impedance of the signal generator; remembering that the output impedance will be resistive, this imposes an accuracy of $\frac{1}{8}$ per cent. In the same paragraph a statement occurs with which I cannot agree. It reads: "It is essential that the shunt capacitance reactance of the coil and its connections shall be large compared with the inductive reactance." To get a minimum of error, that shunt capacitance reactance must be equal in magnitude to the inductive reactance. In fact, so long as the shunt capacitance reactance is not less than half the inductive reactance the error will be no greater than it would be if there were no shunt capacitance there at all.

Again, when making distortion measurements we are instructed to insert a filter between the audio-frequency oscillator and the modulator. The performance of the filter is specified to be such that the second harmonic of the oscillator is attenuated by at least 80 db. Since the second-harmonic level in the oscillator output circuit can hardly fail to be less than -20 db. relative to the fundamental, the overall accuracy is in this case, by implication, 0.001 per cent!

I should like to express regret that the term "automatic volume control" has been perpetuated in the specification. "Automatic gain control" is more precise, I consider.

With regard to the distortion factor (Part II, Section 1), I should like to ask for an explanation of the factor $\frac{1}{2}$ which multiplies the expression defining this factor.

Finally, Part II, Section 5, states "A curve shall be plotted on plain-ruled cm. graph paper between acoustic output, expressed as a loudness level, and distortion factor." Unfortunately, however, the curve given at the end of the specification expresses an intensity level instead of a loudness level. I should like to know which one of these results really should be expressed. I notice that these tests should be made with a modulation percentage of 60, a figure which I regard as undesirably low. It would, for example, fail to reveal the obvious superiority of a diode as compared with a leaky-grid detector.

Mr. E. W. D. Negus: I do not consider that the specification covers every measurement necessary for a complete knowledge of a radio receiver, and I think that the methods of making measurements should be laid down very precisely. For example, I should like to see the measurement of padding errors included in the specification. One of the difficulties encountered when taking padding curves is the necessity to tune a circuit which has an asymmetric response. There are various ways of tuning such a circuit, but all are compromises. The thought induced by new methods of measurement may lead to considerable improvements, and the time would appear to be ripe for a series of carefully-prepared discussions upon the difficulties met with in making certain measurements, because it is apparent that few workers assess the same circuit in the same way.

For the measurement of selectivity I agree with Mr.

Alford that it is undesirable for many reasons to use two standard signal-generators. Selectivity is required in a receiver to stop an unwanted station interfering with a wanted station, and the interference obtained I divide into two sections. One I call "side-band splash," and this occurs at frequencies from 1 000 to 3 500 cycles per sec. The other, for which I reserve the name "monkey chatter," occurs round about 500 cycles per sec. This is a purely personal division, which I have not seen stated previously. Comments on it would therefore be interesting.

Mr. Scroggie objected to the use of 200 kc. as one of the measuring frequencies, because he had met with interference from Droitwich; I would inform him that I have had no difficulty at all with interference from Droitwich at a place where the field-strength measured in the open is 20 millivolts per metre.

Mr. S. Hill (in reply): I agree with Dr. Smith-Rose that the time is ripe for a comprehensive specification sponsored by the British Standards Institution. The Radio Manufacturers' Association has borne this in mind and intends, when satisfied that the present specification has reached a stable form, to present it to the B.S.I. as the basis for such a comprehensive specification. Mr. Scroggie thinks that this work is likely to take a long time. This is a good reason for issuing the R.M.A. specification immediately so as to tide over the period of delay.

Mr. Scroggie notes that difficulties may arise with the 200-kc. measurement. It is expected that where such difficulties arise the manufacturer will find a solution which adheres to the spirit of the specification. In this instance a note will be added permitting the use of convenient adjacent frequencies. Where a smooth curve is drawn, additional frequencies must be used in any case, and the difficulty hardly arises.

The reason for limiting certain measurements to long and medium wave-bands is that the technique of short-wave measurements and the apparatus available were not considered to be sufficiently advanced at present. It may be feasible to include short waves in later issues.

The suggestion for measuring selectivity in decibels is quite sound, although it is perhaps open to the logical objection that decibels imply, by definition, a ratio of power, whereas we are here concerned with voltages only, the input impedance not being explicitly concerned. However, the definition given was thought to be more acceptable to manufacturers and also to bear the simplest relation to what interests the user, namely the strength of interfering signal which can be tolerated without audible confusion.

The point about the use of an r.m.s. meter for noise measurement is also sound. The definition really implies the use of such a meter, but in case of hardship the use of a rectifier instrument would be tolerated as not likely to introduce serious error.

It is agreed that there may be appreciable audible components of hum above 100 cycles per sec., but it is highly improbable that these would affect the meter visibly.

In his remarks on A.V.C. and Q.A.V.C., Mr. Scroggie opens a controversial question. The A.V.C. test quoted was primarily framed to cover the performance of an

A.V.C. system under the conditions which most often apply, viz. the reception of signals of the order of 1 mV, which will be attractive by being free from interference, but may be marred by fading. Any test intended to cover all the design points would have to go much further even than Mr. Scroggie's suggestion. The form of curve suggested by Mr. Scroggie has been considered but is felt to be open to objection. Admittedly the R.M.A. test calls for the measurement of a smaller output (though it is of the same order as that given by the proposed method), but both methods necessarily lead to a range of outputs which is too wide to be measured on a single instrument. The R.M.A. prefers to extend the range of the meter by the conventional method of scale multipliers, believing this to be much less vague than the process of resetting the receiver controls advocated by Mr. Scroggie. The point about the 1-volt output of signal generators is worth further consideration.

No test is proposed for Q.A.V.C., because this is only one of several methods used to achieve "quiet tuning." Other common methods are the use of limitation of the ultimate sensitivity, and the use of muting circuits controlled by selective discriminating circuits; there are also various mechanical methods. It would be difficult to devise tests which would distinguish these devices from the user's point of view, and the Committee therefore preferred to concentrate on tests directed towards the more important parameters of the receiver.

The specimen A.V.C. curve should, for consistency's sake, show an ordinate scale of watts, as Mr. Scroggie suggests, rather than of volts.

It is agreed that "equally as loud as" is probably scientific jargon rather than good English. I have striven to achieve clarity rather than an academically correct style; nevertheless, slovenly writing is to be deprecated and, if good dictionary authority can be found to support Mr. Scroggie's contention, the phrase will be amended. It is, of course, agreed that the loudness comparison of sounds of different characters is a matter of great difficulty. Harvey Fletcher and other workers, however, have made these comparisons once and for all and issued frequency-loudness-intensity curves which are generally accepted. These curves have been used in designing the weighting of the amplifier, so that no further subjective judgments are involved in the tests.

The difficulties of using signal generators in series are appreciated, but a detailed discussion of how to deal with such difficulties would, I think, be out of place in the specification and difficult to make comprehensive. In specifying the method the R.M.A. relies on the experience of its members who have applied the specification successfully to commercial receivers.

The multiplicity of frequencies commonly used as standards is a great pity. I can only say that these different frequencies have been used as sparingly as possible. There seems to be no hope of standardizing weighting curves, as each weighting is really only suitable for one type of sound and one loudness. Hum in a radio receiver is so specialized a noise that it is unlikely that there will be any clash with such a body as the C.C.I. or with those bodies concerned with measuring industrial noises.

The 3 % of 5th harmonic is certainly arbitrary, although

it bears some relation to analyses of the various mains supplies available to the manufacturers who drew up the specification. It is not to be thought that 5th harmonic is specially prevalent. It was included to detect a particular type of hum often present in radio receivers and missed by 50- or 100-cycles-per-sec. measurements, namely the type of hum introduced by pick-up, within the receiver, by an unscreened grid lead. Lower harmonics than the 5th would not show up this defect, and higher harmonics are not sufficiently prevalent. Dr. Whitehead hopes to see the question of reduction of interference dealt with in later issues. The R.M.A. feeling, however, is that the question is best dealt with in an entirely different document and in co-operation with other bodies.

The average listening level of 60 to 74 phons quoted by Mr. Pawley is probably about normal; our own tests seem to suggest about 65 phons. The figure of 50 phons was chosen as a round number which would certainly not cause overloading in any receiver. It has much the same status as 50 milliwatts in the electrical tests. We hold that 80 % modulation is inconveniently high in the present state of the art of manufacturing signal generators. The R.M.A. is aware that such depths occur in practice, but it would like to see administrations accepting about 30 % as an average depth of modulation.

The formula given in Part 2, Section 3, appears more fearsome than it really is. Thus the expression $10^{(I_w - I_0)/10}$ is the conversion of a difference in decibels ($I_w - I_0$) to its corresponding power ratio. It may be evaluated at once from a table of antilogarithms or by a single setting of a slide-rule. To repeat a portion of the test, as suggested, would be a more lengthy procedure than to make a slide-rule calculation.

It must be admitted that the various subjects mentioned by Mr. Pawley ought to be included in an ideal specification. The R.M.A. specification does not claim to be ideal. As pointed out in my opening remarks, it is written by manufacturers for manufacturers and, although criticism is welcomed and has been of use in modifying some of the detail, the general outline remains and represents all that the manufacturers are prepared to do at present.

In reply to Dr. Hughes the figure of 50 mW is certainly low, and it was specified because it is an accepted figure. Where it causes any difficulty, the specification permits measurements at higher levels together with a linear reduction. I cannot agree that battery sets with an output of less than 400 mW are rare. Receivers with an output of about 200 to 250 mW are very common.

Dr. Hughes asks what percentage of distortion is taken in assessing the output of a receiver. The specification has carefully avoided quoting any such figure: it is concerned only with the methods of measurement and the expression of results, not with performance limits. It is true that the nominal output of the receiver is referred to, but the margin allowed is so wide that no difficulty is likely to result whatever system of arriving at the nominal output be used.

Dr. Hughes is surely misinformed about the definition of the British Standard phon. The figure is 0.204 millidyne. It is agreed of course that the difference between

this and Dr. Hughes's figure of 0.200 millidyne is negligible in practice.

Dr. Hughes's figures for practical loudness-levels do not accord with the Committee's experience. Measurements have been made in school class-rooms under a variety of conditions and for male and female voices. An average of 65 phons seems to be a suitable figure for voice levels. No tests were carried out on music, but I suggest that a level of about 80 phons would be normal. Dr. Hughes agrees with this latter level of 80 phons but seems to state that his voice (84 phons) is louder than an orchestra. Actually a real orchestra would be much louder than this. I have heard Tschaikowsky at something approaching the threshold of feeling!

In reply to Mr. Smyth there are difficulties about the use of one-quarter of the nominal output of a receiver in place of 50 mW. Firstly the nominal output is a figure which is not definite to within 50 %, and, secondly, the sensitivity figures would include A.V.C. performance and so give a misleading result.

The method of selectivity measurement proposed by Mr. Smyth is supplementary to the one specified. If extreme selectivity were obtained in the tuned circuits of a receiver and the frequency characteristics were compensated in the audio stages, the R.M.A. method would indicate a high selectivity corresponding to the fact that no speech or music would be heard from an adjacent station, whereas Mr. Smyth's method would indicate a much lower figure corresponding to the fact that there might be an appreciable monkey-chatter. It is debatable that the selectivity of a receiver should involve both measurements. The compilers, however, were unwilling to have two selectivity figures and, in choosing the method they specify, they were expressing the view which they believe is generally held by the public, namely that music-with-the-news is the more objectionable result of poor selectivity. "Monkey-chatter" can be dealt with, to some extent, by using the tone control, but, except in reaction sets, which are excluded from the R.M.A. specification, the other type of selectivity is in the hands only of the manufacturer. It may be added that for many listeners (such as, for example, the schools) selectivity means the ability to clear the London Regional of the National Programme. The beat-note method does not deal with this problem.

Mr. Gill's remarks suggest that the title of the R.M.A. specification should be changed from "... Radio Receivers" to "... Radio Broadcast Receivers." This suggestion is accepted by the Committee. Perhaps when the B.S.I. draws up the comprehensive specification, referred to earlier, it will be possible to extend the range to cover other than broadcast receivers.

Mr. Gill refers to the question of I.F. response. The Committee at one time drafted a section dealing with spurious responses. There were, however, so many of these, and it was obviously so invidious to omit any of them, that it was decided not to include spurious response at the present.

In reply to Mr. Barnard, phase-shifts are, no doubt, of growing importance. At present, however, only a small minority of users is interested, and it is felt that the time is not ripe for treating them. A user with a special interest in phase-shift would, no doubt, study the

problem on two or three types of receivers, by means of a cathode-ray oscillograph, and choose that type which best met his special requirements.

Mr. Barnard should note that if phase-shift were eliminated altogether from the receiver, it would still creep in at the loudspeaker. Of course, the question becomes of more importance for television, but the specification is not intended to cover television receivers.

Mr. Alford's remarks are of great interest, since they represent a viewpoint necessarily different from that of radio broadcast receiver manufacturers. Mr. Alford certainly shows how wide a comprehensive specification would have to be. I only claim that we have laid down a skeleton and filled in such of the detail as is of interest in our own field. No doubt, allied specifications could be written by other interested bodies with applications in special fields. A specification which embraces all radio receivers may perhaps never be written. This is why we propose to adopt Mr. Gill's suggestion and strictly qualify the words "Radio Receivers" in the title.

As a result of Mr. Holt Smith's criticism of the accuracy limits imposed, the Committee has carefully reconsidered these and has changed some of them. Others, however, stand in spite of the criticism. Thus there is no real inconsistency in quoting a limit of 10 % for the signal generator in Part 1 and 20 % in Part 2. This merely means that for the measurements of Part 2 it is not necessary to bring out the best signal generator.

Again, although a figure of 20 times is mentioned, in parenthesis, for the ratio of the coil impedance and generator impedance, all that is really specified is that either open-circuit conditions may be assumed to hold or a suitable load correction shall be applied. If source and load are truly in quadrature it is true that open-circuit conditions apply with a relatively small ratio. Twenty times would only be necessary in the extreme case of a generator which, with its connections, was mainly inductive. Another instance in which Mr. Holt Smith has read into the specification more definiteness than was really intended is the attenuation of the low-pass filter. The attenuation of "at least 80 db." was the test figure of the actual filter used in the tests. It is only claimed for this circuit that it is one of many possible circuits having the required characteristics.

Mr. Holt Smith's contention that shunt capacitance across the coil will not introduce error is, of course, correct if it can be assumed that the capacitance is lumped. The Committee had in mind the possibly disturbing effect of distributed capacitance. However, the case for retaining the warning is not very strong and it has since been omitted.

Mr. Holt Smith is, however, quite right about the imposed accuracy of 0.5 db. for microphone and amplifier. The intention was to correct the device to within 0.5 db. on the assumption of a known microphone characteristic. This is not clearly expressed, however, and the statement will therefore need correction.

The term "automatic gain control" is certainly more accurate than "automatic volume control." For that matter, the knob known as the "volume control" should also be called a "gain control." The Committee, how-

ever, sees no likelihood of the term "A.G.C." becoming popular, nor does it feel it a duty to reform the standard nomenclature of receivers and components. One might waste much energy deciding between such terms as "valve" and "vacuum tube"!

The factor " $\frac{1}{2}$ " introduced into the definition of distortion factor has somewhat the significance of the factor "10" introduced into the definition of the ampere. It is intended to make the range of values dealt with in practice compare with the percentage figures hitherto used, thus minimizing the shock which users might feel in changing from one standard to the other. This may be heresy academically, but we are concerned with the practical acceptance of the specification by manufacturers.

It is true that the present distortion (acoustic output) test takes no account of the distortion produced in the radio-frequency and detecting stages of the receiver. But the test proposed by Mr. Smith would introduce harmonics produced in the radio-frequency and the audio-frequency parts of the receiver with a random phase relationship, and would result in confusion.

Some members of the R.M.A. have sought to overcome the difficulty by carrying out the following tests:—

(a) The test called for in the specification, to show the audio-frequency distortion;

(b) A test under similar general conditions, but with low and constant audio-frequency output, the depth of modulation being varied; and

(c) A repetition of test (b) for a range of radio-frequency inputs.

These tests give a more complete description of the receiver than does the R.M.A. test, but the complexity of the tests and the scarcity of suitable signal generators force the Committee to be content with the present specification, which, after all, is mainly intended to combat the normal meaningless and often untrue claims of "undistorted watts output."

In reply to Mr. Negus it is agreed that there are many omissions in the specification. Some matters have not been treated because of difficulties of measurement or interpretation; others because of the difficulty of being sufficiently exhaustive; others again—and padding errors are probably in this class—because they do not seem important factors, from the users' point of view, in assessing the overall performance of a type of receiver. Manufacturers should take care of such matters as part of their routine testing practice.

It may, perhaps, be emphasized again that the specification is presented largely as a piece of information showing what the manufacturers intend to do. Possibly other bodies may be moved to write similar documents covering some of the matters we have not treated. Probably, also, the scope of the present specification will be enlarged as experience is accumulated.

Mr. Negus points out that there are at least two types of selectivity. The feeling of the Committee on this point is expressed in the reply to Mr. Smyth.
